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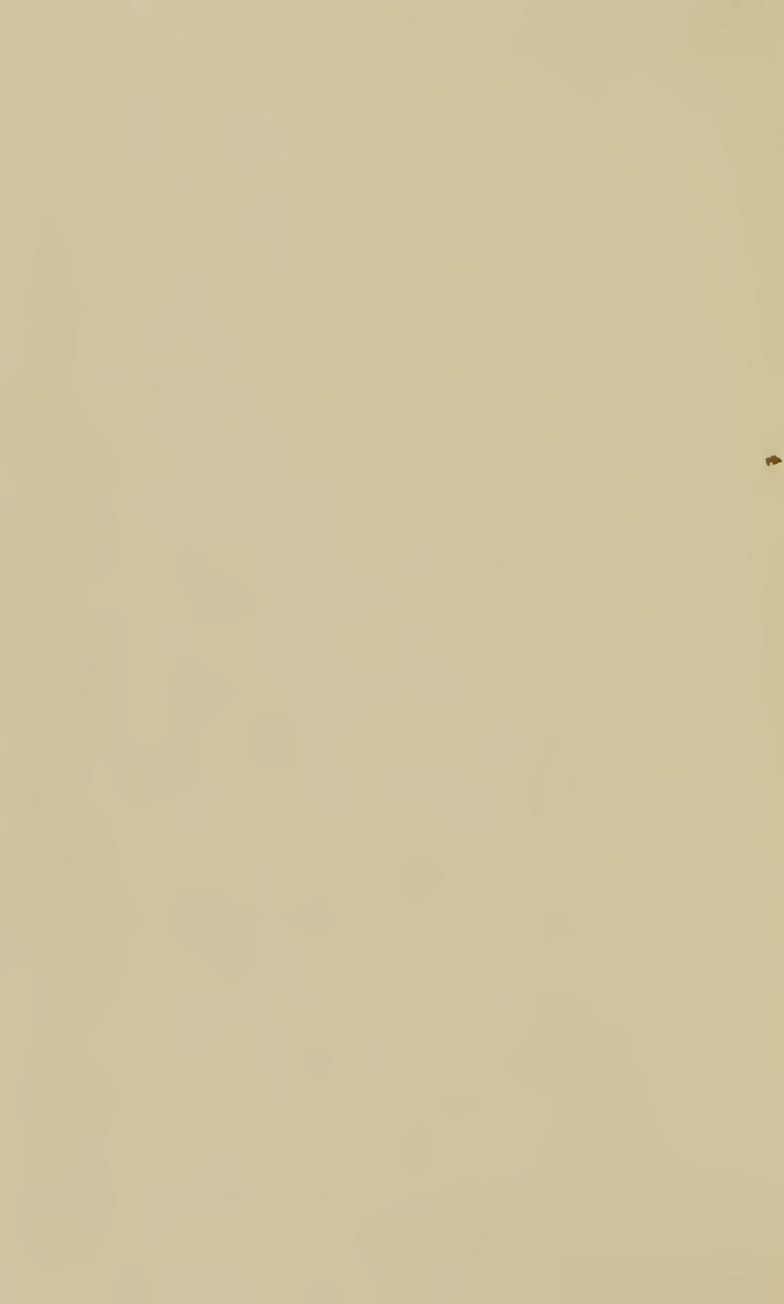
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AN
EXPERIMENTAL ESSAY
ON THE
CONVERSION
OF
CHYLE INTO BLOOD:

BY JAMES HUTCHINSON, A. M.

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“ Simple Anatomy is a mass of dead matter. It is Physiology
“ which infuses life into it.”

RUSH on Animal Life.

1487
Philadelphia:

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.....
1803.

AN
EXPERIMENTAL ESSAY
ON THE
CONVERSION
OF
CHYLE INTO BLOOD:

SUBMITTED TO THE EXAMINATION
OF THE
REV'D. JOHN ANDREWS, D.D. PROVOST,
(*PRO TEMPORE*);

THE
TRUSTEES, AND MEDICAL PROFESSORS,
OF THE
UNIVERSITY OF PENNSYLVANIA;

ON THE 8th OF JUNE, 1803.
FOR THE DEGREE OF DOCTOR OF MEDICINE.

TO

JOSIAH HEWES,
SAMUEL COATES,
PATTISON HARTSHORNE,
ELLISTON PEROT,
LAURENCE SECKEL,
THOMAS MORRIS,

JAMES SMITH, junr.
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PHILIP SYNG PHYSICK, M. D. } Surgeons.
JOHN REDMAN COXE, M. D. }

OF THE PENNSYLVANIA HOSPITAL,

THIS ESSAY

IS INSCRIBED,

AS A MARK OF

RESPECT, GRATITUDE, AND ESTEEM,

BY THEIR MUCH OBLIGED

FRIEND AND PUPIL,

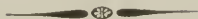
THE AUTHOR.

TO
CLEMENT BIDDLE, Esq.
THIS ESSAY
IS INSCRIBED,
AS A SMALL, BUT SINCERE,
TRIBUTE OF GRATITUDE AND ESTEEM,
FOR NUMEROUS FAVORS CONFERRED ON
HIS MUCH OBLIGED FRIEND,
AND AFFECTIONATE NEPHEW,
THE AUTHOR.

“ Thus where the veins their confluent branches bend,
And milky eddies with the purple blend ;
The chyle’s white trunk, diverging from its source,
Seeks through the vital mass its shining course ;
O’er each red cell, and tissued membrane spreads
In living net-work all its branching threads ;
Maze within maze its tortuous path pursues,
Winds into glands, inextricable clues ;
Steals through the stomach’s velvet sides, and sips
The silver surges with a thousand lips,
Fills each fine pore, pervades each slender hair,
And drinks salubrious dew-drops from the air.”

DARWIN’S BOTANIC GARDEN.

CHAPTER I.



ON THE CONVERSION OF

CHYLE INTO BLOOD.



AS Physiologists have paid but little attention to the subject which I am about to consider, I feel a diffidence in entering upon the investigation of it ; for after many tedious researches into the works of the most eminent authors, both ancient and modern, nothing satisfactory could be discovered on the Conversion of Chyle into Blood. Lower, a very re-

ſpectable Phyſiologiſt, in a Tract publiſhed in 1666, entitled “*Transitus et tranſmutatio chyli in ſanguinem,*” expreſſes himſelf in the following manner. “*Quibus vero modis, et mutationis gradibus chylus in ſanguinem aſſimiletur, et in nutrimentum partium faceſſat, ut melius intelligatur, ſciendum eſt, ſpiritum vitalem, aliaque in ſanguine principia activa in chylum jugiter inſtillatum agere, eumque minutiffime ſubigere; quin et cum chylus ſale, ſulphure, et ſpiritu copioſe turgelſcat, quampri- mum compages ejus à fermentatione laxatur, particulæ iſtæ activæ motus libertatem adeptæ, cum partibus ſanguinis, quæ ſimilis et congeneris naturæ fuerint, ſeſe prompte aſſociant, et in ſanguine, (ut in vino, aliisque ejusmodi liquoribus) contingit, ut ubi ſpiritus dominio potiti ſint, particulas omnes craſſiores, et fæculentiores, quibus impinguntur, à maſſa ſua deturbent et expediant, quo reliquam liquoris partem defæcatiorem et puriorem reddant.*” This account of the manner in which Chyle is converted into Blood is as erroneous as it is hypothetical, though no doubt it was gene-

rally received at the time the author lived ; for he was a man of genius, and a distinguished Physiologist, and for these reasons, his sentiments are still worthy of our notice. The above quotation will also serve as a specimen of the ideas entertained on this subject so late as 1666.

After the time of Lower, the opinions on sanguification generally agreed with those proposed by him, in imputing the changes which the Chyle undergoes, after it passes into the blood-vessels, to a fermentation, or to its being mixed with salt, sulphur, &c.

Since the splendid discoveries of Priestley, Lavoisier, and other illustrious modern chemists, revolutions have taken place in Physiology, as well as in other sciences ; for the discovery of a new truth, as the ingenious Spallanzani observes, opens the way for numerous others, and when the mind has once hit upon a right scent, it cannot wholly pursue its inquiries in vain.

The effects of respiration on the blood have been more attentively examined; and the chyle which passes into the left subclavian vein, and from thence to the right side of the heart, and through the lungs, has been supposed, by the time it reaches the aorta, to be completely converted into blood, fit to nourish the body, and of course to be the fluid from which all our secretions are derived. This is the opinion which is now generally entertained on the Conversion of Chyle into Blood; it is one however to which I cannot subscribe, because my experiments, hereafter to be related, lead me to think very differently.

I propose in as brief a manner as possible, to treat of the formation of Chyle, and then proceed to the more immediate consideration of the subject.

The aliment, which is received into the stomach, is, by means of the gastric juice, and probably by fermentation, converted into

a pulstaceous mafs, which is propelled into the duodenum, and there subjected to the action of the bile, and liquor of the pancreas. The nutritious part, called Chyle, is separated from the fæcal, or excrementitious, and is taken up by the lacteals, called vasa inferentia, which arise from the surface of the small intestines; it passes through the mesenteric glands, and is received from them by another set of lacteals, called vasa efferentia, which finally terminate in the thoracic duct, and there pour out their contents. Now, whether the chyle undergoes any change in passing through the mesenteric glands, is yet undetermined; no difference can be observed in it, after passing through them; for, though I have repeatedly examined it with a microscope, when taken both from the vasa inferentia, and efferentia, yet the same appearances always presented themselves.

That the mesenteric glands assist in forming the globules of the blood is rendered hypothetical from the preceding observation, as

well as from the well known fact, of globules existing in the chyle before it arrives at those glands.

There can be no doubt, but that the chyle must be, in some measure, altered by passing through those glands, because we know that they certainly must answer some useful purpose in the animal œconomy, and none can, with more propriety, be referred to them, than that of producing an alteration in the chyle.

In the thoracic duct, the chyle is mixed with the lymph, and this must also produce a farther alteration; but this part of the subject, like the former, is still involved in obscurity, in the course of my experiments I have made this observation, viz: That the fluid of the thoracic duct, (as I shall hereafter call it), has a greater disposition to coagulate, than the chyle in the lacteals or mesenteric glands, which may be owing to the union of the former with the lymph.

The following are the properties of the fluid of the thoracic duct. It has a sweetish taste ; coagulates almost immediately upon exposure to air ; and when in a large quantity, separates into a serum, and coagulum ; globules much smaller than those of the blood can be discovered in it by a microscope. It contains no fibrina.

To ascertain whether it has those properties of the blood, which are said to be characteristic of vitality in the latter, I made the following experiments.

EXPERIMENTS.

No. I.

a. A quantity of the coagulable part of the blood was subjected to Galvanic influence, and, by means of a microscope, contractions and oscilations were plainly observed, similar to those which the fibres of a muscle exhibit, when in the same situation.

b. The fluid of the thoracic duct, recently coagulated, was exposed to the same influence, but no such phenomena were exhibited.

c. A small quantity of blood was obtained from a vein, I received it on the back of my hand; in a few minutes a thin layer of coagulum was formed, which was immediately applied to an ulcer; a piece of oiled linen was placed over it, and the whole secured moderately tight with a bandage. It was examined in twelve hours afterwards; and I observed that it adhered, in many parts, to the ulcer. In twenty-four hours the adhesion was more complete, and those parts, which had not united, were become quite putrid; I therefore removed them. In twelve hours more the ulcer was again examined, and the surface appeared very uneven, though perfectly healthy; the granulations being much *more* eminent, where the coagulum had adhered.*

* This experiment was performed in the summer of 1801, with a view of ascertaining the manner, in which the cavities made by ulcers, are filled up. From the result of it, I was satisfied, that the coagulable lymph of the blood was the substance from which new parts are formed. I think it also proves to a certainty the vitality of the blood.

To perform this experiment with success, an ulcer must be selected, with firm and florid granulations, discharging a thick yellow pus. If the granulations be soft and spongy, the experiment will not succeed. Before the coagulum is applied, the surface of the ulcer must be made as clean as possible.

d. A quantity of the fluid of the thoracic duct, recently coagulated, was applied to an ulcer, of the above mentioned appearances; but the result was very different; no union between it and the ulcer took place.

From these experiments we may infer, that the fluid of the thoracic duct has not those properties of the blood, which are said to be characteristics of vitality.

From the thoracic duct, this fluid passes into the left subclavian vein, and is conveyed to the right side of the heart. We now find it in a new set of vessels, and circulating with the blood. From the right ventricle of the

heart, it passes into the pulmonary artery, and through the lungs; it is then received by the four pulmonary veins, and conveyed to the left auricle: It now passes into the left ventricle, and from thence to the aorta. Here this fluid has been traced, and no farther; and by the time it arrived at the last mentioned place, has been supposed to be converted into blood. The whole process of sanguification was now thought to be finished. The great alteration, produced in the fluid of the thoracic duct, was attributed to the absorption of oxygen in its passage through the lungs.

Let us attend to the changes, which it is possible for it to undergo, as it passes from the right to the left side of the heart.

The fluid of the thoracic duct, by being mixed with venous blood, in the superior cava, right auricle, and right ventricle of the heart, has a large surface exposed to be acted upon, by the air in the lungs.

The atmosphere, which we breathe, is composed of two very different gases, viz. oxygenous, and azotic ; and generally contains a very small portion of carbonic acid. This last however is considered as a foreign matter, and not a necessary ingredient. According to modern Chemists and Physiologists, in respiration, the oxygenous part of the air is absorbed by the blood in the pulmonary vessels, and unites with the phosphorus that it contains, forming phosphoric acid, which combines with the iron of the globules, and gives them their red colour.. Mr. Davy believes, that a portion of azote, as well as oxygen, is also absorbed, though this is doubted by several very eminent Chemists. Water in the state of vapour, and carbonic acid, are evolved from the pulmonary artery ; but whether a small portion of both, or either of them, comes from the fluid of the thoracic duct, we are uncertain. The effect produced on the blood by this alternate absorption, and emission of air, is a change in it from a dark purple to a florid colour.

The fluid of the thoracic duct, whilst circulating with the blood in the lungs, is also exposed to the action of the air; and one great use of respiration is, that it should assist in sanguification. But we are told that, the fluid of the thoracic duct cannot be seen in the blood, after it has passed through the lungs. If that be the case, it must be changed into blood in the lungs. But this I deny. For, I have seen it in blood drawn from the aorta, as often as in that from the pulmonary artery. This, and other facts, induced me to doubt the common opinion on this subject. I shall relate them as they occurred to me.

EXPERIMENTS.

No. 2.

a. A dog, that was fed at twelve o'clock, A. M. had his aorta opened at four, P. M. Four ounces of blood were obtained from it, and received into a porringer. While it was coagulating, I observed it with great attention; but could perceive nothing but the common

appearances of blood, when in that state. It was examined an hour afterwards, when it had separated into serum and crassamentum; several white streaks were seen in the former, and a firm white coagulum on top of the latter. In two hours it was examined again, and I thought the number of white streaks had increased.

b. A dog, that was kept fasting for twenty four hours, had eight ounces of blood taken from his aorta; it was examined, when coagulating, and several times afterwards, but nothing uncommon was observed.

c. A dog was fed at eleven, A. M. and had an artery in his fore leg opened at three, P. M. Eight ounces of blood were received from it into a basin. I examined it, when coagulating, and nothing unnatural was seen. In the course of two hours, it was examined again, and a white coagulum was observed at the edge of the crassamentum. The serum had a slight wheyish colour.

d. A dog was kept eighteen hours fasting, and had the same experiments performed on him as the last, but the blood exhibited no such appearances,

The above experiments were several times repeated, and their results were generally the same. I must confess that in every instance, I was not so fortunate, as to find the appearances which have been described; but at the same time I must observe, that they occurred, at least, in the proportion of five out of six.

It is curious that the fluid of the thoracic duct could never be seen, until the blood had coagulated; and it was always more apparent, after the latter had separated into serum, and crassamentum. To ascertain, whether the fluid of the thoracic duct could be mixed with the blood, without being evident to the senses, I poured about one drachm of the former whilst fluid, into two ounces of the latter fresh from a vein. I stirred them together several times. They were examined about five mi-

nutes afterwards, and the whole mass was of a red colour; no white spots, streaks, or coagula could be seen. In two hours, the appearances were very different; for I observed the fluid of the thoracic duct coagulated on the crassamentum. I relate this experiment merely to shew, that the detection of the fluid of the thoracic duct in the blood, is not owing to its being unmixed with the latter, but to its being unaffimilated.

From having so repeatedly detected the fluid of the thoracic duct in the blood, after it had passed through the heart and lungs, I conclude, that the opinion supposing sanguification to be completed in the heart and lungs, is erroneous and hypothetical.

There will, perhaps, be some objections to this inference, as it will lead to a theory very different from that which is current at the present day. Those, however, who are supported by experiment cannot be considered as too confident in the delivery of their sentiments.

Our object is truth and the first step towards a knowledge of that is the abolishing of error.

The only objections, which occur to me at present, that can be urged against the conclusions which have been drawn are the following, 1st. That the appearance of the fluid of the thoracic duct, in the blood of the aorta, was morbid, and might have been owing to an increased action of the heart, and arteries of the animal, when subjected to such a severe experiment, propelling their contents too rapidly through the lungs. This, at first, struck me very forcibly. But, after having made experiment No. 2, and attended to its result, where the artery could be opened without putting the dog to any great pain, or much increase of arterial action, I was satisfied of the correctness of the conclusion. 2dly. That the appearance of the fluid of the thoracic duct, in the blood, was accidental. This objection is of no weight whatsoever. Any one may be convinced of this, by the results of the experiments being always similar. And, if we

only attend to the flow manner, in which the fluid of the thoracic duct gets into the left subclavian vein, viz. drop by drop, we certainly would not ascribe the results of the experiments to accident.

Though I have denied that fanguification is completed in the lungs, yet I have many reasons for believing, that these viscera give great assistance in the process. But, what precise change is made in the fluid of the thoracic duct, by passing through them, we are not able to determine ; as the former never has been seen, until coagulated ; and microscopical experiments made on it in that state, would be of no service to us. We must therefore rest satisfied with our knowing, that whatever change is produced in it, by passing through the lungs, must be attributed to the absorption of oxygen, and the action of the blood-vessels.

Let us follow the fluid of the thoracic duct, and see if it can be detected in venous blood.

EXPERIMENTS.

No. 3.

a. Having fed a dog at noon, I opened a vein in his thigh at four, P. M. and obtained about eight ounces of blood. Nothing remarkable was observed, during the coagulation of it. I examined it about two hours afterwards, and found the serum of a slight white colour, but no white spots, streaks, or coagulum was observed.

b. The same animal was kept twelve hours fasting, and lost about eight ounces of blood from the same vein. But the appearance of it, both before and after coagulation, was quite natural, the serum being transparent.

c. Equal quantities of the serum, from each of the above different portions of venous blood, were poured into two separate vials, and set aside in a closet. Two days afterwards, they were examined; and I found the transparent

serum had an offensive smell, and every mark of being putrid. The white serum was perfectly sweet. From this last experiment I have not the least doubt, but that the white colour of the serum is owing to the fluid of the thoracic duct. Hewson and Cruickshank are both of opinion, that chyle may exist in venous blood; though the former believes the milky colour of the serum to be owing to fat. To the testimony of these respectable Physiologists, I beg leave to add that of Professor WISTAR, whose familiar acquaintance with subjects of this nature renders his evidence of great weight; he informed me, that he had observed the serum of his blood to appear like whey, in several instances in which he had been bled two or three hours after eating. A cream like substance formed on the surface of the serum, after keeping it two or three days.

I have frequently found the same appearance, in the serum of persons who were bled a few hours after eating.

Although I believe this wheyish colour of the serum to be owing to the fluid of the thoracic duct, yet by it I do not mean that thick and milky appearance which we sometimes find in the blood; but I mean a faint white tinge in it not unlike to lime water, rendered slightly turbid by carbonic acid.

Before this time the reader will have observed, that the appearance of the fluid of the thoracic duct in venous blood, differs materially from that found in arterial. The former seldom being seen coagulated, or in streaks, but generally giving the serum, an uniformly white tinge, as mentioned above,

Here we find that a great change is made, on the fluid of the thoracic duct, by the time it has passed from artery to vein; but in what manner the alteration is produced, cannot be so easily determined. Whether the arteries and veins effect it separately, or conjointly; or whether it is produced in the termination of arteries into veins, is yet to be

ascertained. Possibly, a part of the oxygen, which the blood received in the lungs, may be imparted to the fluid of the thoracic duct. Could not the blood have the power of assimilation?

As we observed that the alteration is made in the fluid of the thoracic duct, before it had reached the veins, so we may reasonably infer, that it must have been done in the arteries. But in what manner I have only ventured to conjecture; leaving it to persons better qualified than myself to decide.

Having ascertained, that a great part of the process of sanguification is done in the lungs, and arteries, and that from the appearance of the serum of venous blood, it is not there finished, I thought that what still remained to be effected, must be done before it was returned to the heart. I therefore paid more attention to the veins, and performed the following experiments.

EXPERIMENTS.

No. 4.

a. A dog was fed at eleven, A. M. and had his cava ascendens just above the junction of the two iliac veins opened at three, P. M. Eight ounces of blood were received from it into a basin, and set aside to coagulate. I examined it in about three hours; and found the serum to have a very slight white tinge, much less evident than that observed in the experiments under the head of No. III.

b. A dog was kept twenty-four hours fasting, and had the cava opened in the same place, with the animal which was the subject of the preceding experiment. Eight ounces of blood were received from it. I examined it about two hours after it had been drawn, and found the serum to be perfectly transparent.

The cavæ of a number of other dogs were opened in different places between the junc-

tion of the two iliacs, and superior part, just where it joins the right auricle of the heart. In all of these experiments, I observed the whiteness of the serum to be greatest, in proportion to the distance that it was taken from the heart, and gradually to disappear near the upper part of the cava. From these facts we are certain, that the veins have the least to do in sanguification, and that the fluid of the thoracic duct is perfectly converted into blood, before it returns to the heart; but, as yet, it is not fit to nourish the body, being only made into venous blood; for, it must pass through the lungs, and part with a quantity of carbonic acid, and aqueous vapour, and then receive a supply of oxygen, after which it goes into the left side of the heart, and is from there propelled to the different parts of the body.

It is remarkable, that the white serum should be seen in the beginning, and not in the termination of the veins. To what are we to attribute this? We know of nothing to effect

this alteration, but the veins themselves, and the blood they contain. Perhaps, the blood may have the power of assimilating the fluid of the thoracic duct, to its own nature. As we believe in the vitality of the former, it is easily conceivable. We know that it is a property of it, when effused, to be converted into a substance of a similar nature to that on which it is thrown out; thus, when effused on a muscle, it is converted into muscular flesh; when on a bone, bone, &c. Hence we account for the formation of new parts. The fluid of the thoracic duct, though not endowed with vitality, (for this property is not necessary for this purpose), when mixed with the blood, might, by the power of the latter, and the action of the vessels on it, be converted into blood.

I hope the reader will be satisfied, that the white colour of the serum, and white coagula, taken notice of in some of the preceding experiments, were owing to the fluid of the

thoracic duct, from their being only seen when blood was drawn after the animals were fed, and from the serum remaining so long without putrifying.

CHAPTER II.



OF THE

FORMATION OF THE

DIFFERENT PARTS OF THE BLOOD.



HAVING, in the preceding chapter, ascertained the different changes, which the fluid of the thoracic duct underwent, before it became converted into blood, and the parts of the body, in which those changes appear to be made; we will now attempt to account for the formation of the different parts of the

blood, viz. the serum, fibrina, and red globules. This is the most difficult part of the investigation. Numerous difficulties present themselves to forbid success: But having gone so far into the subject, it is necessary to proceed in the inquiry; though I should fail in the accomplishment of my object.

Of the formation of the serum little can be said, and perhaps that will be unsatisfactory. We formerly observed, that the fluid of the thoracic duct, when coagulating, separated into two parts, viz. a serum, and coagulum. From the great difficulty of obtaining the former in sufficient quantities, experiments could not be made on it, to see whether it differed from the serum of the blood. They both agree in the following respects, viz. they are transparent, and are coagulated by heat, acids, and alcohol. But whether the serum of the fluid of the thoracic duct, agrees with that of the blood, in other circumstances, I am unable to tell. From this short and imperfect account of the

fera of each of the two different fluids, no correct conclusions can be drawn. It would, therefore, be both unphilosophical and improper, to enter into any speculations, about the formation of the serum of the blood; for, from the few experiments we have made, it does not seem to differ from that of the fluid of the thoracic duct, and as far as we know, it may be that unaltered.

Of the Formation of the Fibrina.

WE will now say a few words on the formation of one of the most important parts of the blood, viz. the Fibrina; which is justly supposed to supply the waste of the muscles, and many other parts of the body.

We formerly proved, that it did not exist in the fluid of the thoracic duct, and therefore believe it to be formed in the blood vessels.

From the appearances of the serum, mentioned under the head of experiment, No. 3

and 4. we have every reason to believe, that this portion of the blood must be formed in the arteries and lungs. In page 15 we observed, that the fluid of the thoracic duct, having a great resemblance to the fibrina of the blood, we may reasonably suppose the latter to be made from the former. But how it is made, will be difficult to ascertain. Whether it is done by the absorption of oxygen in the lungs, the action of the arteries, or a power of secretion, which the latter may possess, in their extremities near their termination in veins, or by other causes which are unknown, still remains to be discovered.

A very ingenious theory, on the formation of fibrina, has been lately published by Dr. Thomson of Edinburgh, in the fourth volume of his *Elements of Chemistry*, which I here lay before the reader, in the author's own words. "It follows, from the experiments of Fourcroy," says Dr. Thomson, "that fibrina contains more azote, and less hydrogen and carbon, than any of the other

ingredients of the blood, and consequently also than any of the ingredients of the chyle. In what manner the chyle, or part of it, is converted into fibrina, it is impossible to say: we are not sufficiently acquainted with the subject, to be able to explain the process. But we can see at least, that carbon and hydrogen must be abstracted from that part of the chyle which is to be converted into fibrina; and we know, that these substances are actually thrown out, by respiration. We may conclude, then, that one use of the air absorbed, is to abstract a quantity of carbon and hydrogen from a part of the chyle by compound affinity, in such proportions that the remainder becomes fibrina: therefore one end of respiration is to form fibrina. Doubtless the other ingredients of the blood are also new modified, though we know too little of the subject to throw any light upon it." This theory of Dr. Thomson seems to me as probable as it is ingenious, and I adopt it without much hesitation. It clearly appears, that the fibrina must be formed in the arteries near

their termination in veins, where we suppose that a portion of carbon and hydrogen, is abstracted from the coagulable part of the fluid of the thoracic duct, by the oxygen which it received in the lungs; carbonic acid, and water are formed, which go to the venous blood, and are afterwards evolved by it.

Of the Formation of the Red Globules.

Hewson and Falconar thought they had proved, that the red globules of the blood were formed by the spleen, and lymphatic glands; but as the opinions of these gentlemen are now in a great measure neglected, it is hardly necessary to take up any time to refute them. We sometime ago observed, that the fluid of the thoracic duct contained globules of a white colour, smaller than those of the blood. Now, for the same reason that we believed the fibrina of the blood to be formed from the coagulable part of the fluid of the thoracic duct, we believe the red globules of the former, to be formed from the white globules of the latter.

These globules we supposed, in their passage through the lungs, to absorb a portion of oxygen, which may unite to the phosphorus they contain, and form phosphoric acid; this combining with their iron gives them a red colour. I was led to doubt the truth of this opinion from the following experiments.

EXPERIMENTS.

No. 5.

a. About one drachm of the fluid of the thoracic duct was obtained, and, before it had time to coagulate, was exposed to the action of phosphoric acid; but no red colour was produced in it. It underwent no other change than that of becoming harder, and slightly yellow.

b. A quantity of the fluid of the thoracic duct was exposed to atmospheric air, and oxygenous gas; but no change was produced in the colour of it. These facts seemed to mi-

litate against the opinion, of the red colour of the globules of the blood being produced by oxygen; but, when we reflect, that in these experiments the fluid of the thoracic duct was out of the body, no conclusion can be drawn from them.

What renders the idea of the red colour of the blood, being produced by phosphoric acid, more certain is, the existence of phosphorus in the fluid of the thoracic duct. Now we know that these globules do absorb oxygen, as they pass through the lungs; and it has a greater affinity to phosphorus than any other combustible substance which the fluid of the thoracic duct contains; therefore, phosphoric acid is formed, which may unite to the iron of the globules, and give them their red colour.

CHAPTER III.



OF THE MILKY SERUM.



IN this chapter, we shall endeavour to refute the opinion of the late Mr. Hewson, on the cause of the milky appearance of the serum, which is sometimes met with.

Mr. Hewson says, the milky serum contains globules, smaller than those of the blood, and that the former are spherical, and the latter flat. That this serum, when inspissated, was more inflammable, and less tenacious,

than transparent serum, and had oil oozing from it. That it putrified, and then jellied.

As most of the patients from whom it was obtained, had bad appetites, and were subject to vomiting, he concludes that the white colour was not owing to chyle; and, from the serum being so inflammable when inspissated, and oil being seen oozing from it, he believes it to be owing to fat absorbed, and conveyed into the blood-vessels.

I will now relate the case of a woman, from whom I obtained some of this serum, and then a few experiments which were made on it.

A. B. a mulatto, aged forty-five, or thereabouts, had complained of a head ache, and pain in her bowels, for about two weeks. She had had no menstrual discharge for three months. Her pulse was slow, though tense. Her appetite was good, and she had a stool every day. She consulted a Physician who advised her to be bled, and accordingly twelve

ounces of blood were taken from her arm. About five minutes after the blood had been drawn, part of it was observed to be of a white colour, though the fluid was perfectly red when it came out of the vein. When the separation into serum and crassamentum had been complete, the former was of the colour and consistence of milk, but the latter appeared perfectly natural. She was considerably relieved by the bleeding, but a slight pain in her head still remained. A few days after, she was bled again to ten ounces, and the blood exhibited the same appearances as before.

EXPERIMENTS.

No. 6.

About four ounces of this milky serum were poured into a vial, and set in a closet. I examined it in three days, and found an evident separation of it into two different fluids; one thinner and white, which floated above, a thick and cineritious coloured fluid below. A small quantity of this serum was examined

by a microscope, and several white spots were observed in it, appearing about half the size of a pins head, these I took to be globules, but am not able to say whether they were spherical or flat. A quantity of this fluid, and of transparent serum were kept for about ten days, when the transparent serum, was quite putrid, but the milky perfectly sweet.

Equal quantities of the transparent and milky serum were inspissated. The extract obtained from the latter was of a whitish colour, and more abundant than that from the former. Both of these extracts were burnt. But I could not perceive that combustion went on more rapidly in the one, than in the other; neither could oil be seen oozing from the extract of the milky serum, when exposed to a moderate degree of heat.

From these experiments I am satisfied, that the white colour of this serum was not owing to fat, for if it were so, we could not fail to observe it oozing from the inspissated extract,

when exposed to flow heat. From its remaining so long a time without becoming putrid, it is more likely, that the white colour is owing to the fluid of the thoracic duct.

But why the fluid of the thoracic duct should exist in such large quantities in the blood vessels, I shall not attempt to explain; the subject is of too difficult a nature: It has baffled the efforts of the most skilful physiologists.

F I N I S.

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